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position of the metamorphic rocks which make up the azoic rocks of undetermined geological age, overlying the southeastern angle of Pennsylvania. And that by stream and current actions, perhaps in part by glacial, they were brought into the shape of boulders at a time anterior to the deposition of the sedimentary mica schists.

And it is a fact of interest in this connection that the highly garnetiferous mica schists of this district, are charged with dodecahedral garnets, which probably have belonged to pre-existent rocks, inasmuch as their angles and edges are rounded off, and the crystals reduced to an almost globular form. This is true of the garnets while still firmly imbedded in the mica schists, and applies to the garnetiferous mica schists extending over a wide area.

On favorable report of the Committees, the following papers were ordered to be printed.

Remarks on Dr. Asa Gray's notes on Buckley's new Plants of Texas.

BY PROF. S. B. BUCKLEY.

In the spring of 1862 Dr. Asa Gray had two papers in the Proceedings of the Academy of Natural Sciences of Philadelphia, both of which were reviews of some new plants described by me in the same publication a few months previous. I left Philadelphia prior to the appearance of Dr. Gray's papers, being employed by the Sanitary Commission at Washington to make scientific examinations and measurements of Soldiers for anthropological purposes.—*See Anthropological Investigations of American Soldiers, by Dr. Gould*, lately published by the Sanitary Commission. At the end of the war I returned to Texas, where I have been ever since. I did not see Dr. Gray's notes till August, 1867. I have few Botanical works here, and no Herbarium, and have delayed to notice some points in which I think Dr. Gray has not done me justice, in hopes to be able to have a better chance than I have here, but as time is passing I will offer what facts I have now, leaving others for another opportunity.

During 1859, '60 and '61, I made a large collection of rare plants, in Georgia, Alabama, Mississippi, Louisiana and Texas, which I had boxed and started with for the North prior to the war. These were stopped and destroyed at Lavaca, Texas. They were intended for, and directed to, the Academy of Natural Sciences of Philadelphia.

The few I saved I brought with me, but I found the Herbarium of the Academy not as complete as I supposed. I expected to find all the plants which Nuttall had described, as well as full collections of Wright and other botanists who had explored Texas and other southwestern parts of our country. But these were not as full as I imagined, and the Library was deficient in some works which would have aided me in my investigations. I appreciate these facts more fully now, than I did then, and can understand how very likely it is that I have made some mistakes. There are very few botanists who have not had to regret similar errors under similar circumstances. Indeed the object of this paper is to show that Dr. Gray himself has fallen into error in many particulars in the papers in which he criticises mine. For instance, *Clematis Texensis*, Buckley, Dr. Gray says is his "*C. viorna* var. *coccinea*, Pl. Wr. 2 p. 7, *C. coccinea*, Engelman." It is referred to *C. viorna* with the remark that its "leaves are more glaucous, and the thick sepals of a pure carmine red, very rarely purplish." I do not know that Engelman has ever published his name of *C. coccinea*. I believe all that has been published is in the extract quoted. If, therefore, it is, as I have no doubt the majority of botanists will agree with me that it is, a distinct species from *C. viorna*, my name has the right by priority of publication. It grows in the vicinity of Austin.

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Zanthoxylum hirsutum Dr. Gray thinks but a variety of *Z. carolinianum*. There is a variety I know, and I suppose this is to what Dr. Gray refers. I am very familiar with it. It is abundant in the vicinity of Austin. But this is always small, the largest specimens with a trunk rarely exceeding three inches in diameter. This was a small tree, one foot at least in diameter, and attracted my particular attention by its very peculiar appearance, as I rode horseback in the vicinity of Corpus Christi. Its hairy leaves are a constant and distinctive character. I expect to get specimens again.

Ampelopsis heptaphylla Dr. Gray says is "a small leaved state of *A. quinquefolia*, with some of the leaves 6-7 foliate." They are all, or nearly all, 7 foliate. If *A. quinquefolia* were known to be variable in respect of the number of leaflets on the same plant, there might be room to look for a variety with another number of leaflets. But this species is noted for the regularity with which it bears five leaflets only, both in the north and in the south. It grows in the same locality with my *A. heptaphylla*, constantly with five leaflets only. But this is not all; my plant not only has 7 leaflets almost constantly, but they are smaller than *A. quinquefolia*, and it flowers in cymose panicles at the end of April; while *A. quinquefolia* has compound racemes 3-4 inches in length, and does not open its flowers till the middle or end of June.

Vitis monticola, he says, is *V. rupestris* of Schule. In *Plantæ Lindh.* 2, p. 166, Dr. Gray himself says of *V. rupestris*, "It does not climb, but the stems are upright, and only two or three feet in height." This is right. I am familiar with it. But my *V. monticola* does climb, sometimes to the height of 15 feet. But in addition to this there is nearly two months difference in the time of ripening of the fruit. All the inhabitants of this region readily distinguish them as different things.

Vitis Lincecumii he refers to *V. labrusca*, and says that "the Louisiana specimen (of Dr. Hale) exactly agrees with what we formerly cultivated in Cambridge Bot. Garden as the Isabella grape." The Isabella grape is well known here, yet this is readily distinguished by the people of Texas, who call it the "Postoak grape." It has larger and less lobed leaves than the Isabella. The Isabella has naturally but one short bunch—this is shouldered or branching; the berries drop easily from the stems—these are strongly adherent; the skin is rather thick and the berries comparatively large,—while these are thin-skinned and smaller. The *V. labrusca* is a rampant species, this rarely grows 15 feet, and often bears fruit on bush like specimens, 3-4 feet high. I have studied both species very closely, both before and since Dr. Gray's criticisms, and can have no doubt of their distinctness.

Vitis mustangensis Dr. Gray says "is not the mustang grape of Florida, but is the well known *V. candicans* of Engelman." He asserts further that *V. coriacea*, of Shuttleworth, is a thick-leaved form of it, the *V. caribæa* of Chapman, whether of DeCandolle I am still uncertain." I believe the only description of *V. candicans* published, before my description of *V. mustangensis*, is the following from *Plant. Lindh.* 2, p. 166, where Dr. Gray says, "Under the name of *V. candicans* (N. S.) *Engel. ined.*, I have from Lindheimer, as also from Mr. Wright, Texan specimens of what appears to be a variety of *V. californica*, Benth., with the leaves somewhat less dentate, and more densely tomentose underneath." Again, in *Plantæ Wrightiana*, p. 32, in a note at the bottom of the page, he states, "*Vitis candicans*, *Engel. ined.*, which is also the *V. coriacea* of Shuttleworth, *Pl. Rugel. ex. sic.* from southern Florida, is not the same as *Vitis californica*, Benth., to which I was disposed to refer it in *Pl. Lindh.* 2, p. 166. Perhaps it may be *V. caribæa* of D. C." If this is, as I believe, the only description of *V. candicans* ever published, is Dr. Gray justified in terming it "well known?" Surely Dr. Gray does not own to much acquaintance with it, and makes no allusion whatever to its native name "mustang."

Dr. Gray "warns the reader that mustang is not the name of a town or

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country, as the termination *ensis* would imply, but of a wild horse." I may add that it is *also* the name of a stream along which this grape grows in great abundance.

I saw Lindheimer at New Braunfels in Texas in 1860. He told me that Gray and Engelman, with whom he was in close correspondence, were both uncertain about this grape, and that it was still undescribed. He regarded it as "scientifically unnamed." Under the rules of priority of description, I claim my name.

It is unlikely that my mustang grape is the *V. coriacea* of Shuttleworth, for be it remembered I had travelled extensively in Florida, and should have seen this grape there if they had been the same, but I never saw it in Florida; but the greater probability of this is that Chapman, who had resided in Florida 25 years, describes no such grape as mine, but refers the *V. coriacea* of Shuttleworth to *V. caribæa*, D. C., and further says the mustang of Florida is a form of *V. vulpina*. See Flora of Southern States, p. 71.

Vitis bipinnata, with which name the authority of "Torr. & Gray" is associated, belongs really to Wildenow.

Astragalus Brazzensis, Dr. Gray has, on a second examination, done me the justice to declare a good species.

Hoopesia arborea I am charged with "making up from a flowering specimen of *Cercidium texanum*, Gray, a fruiting one of *Acacia flexicaulis*, Benth, and a sterile branchlet of *Acacia rigidula*, Benth." Of course, no one would suppose I would mix plants purposely with the object of making a new genus or species. That no such a mixture was possible, I claim from the following facts. I was careful to select a large number of specimens with *both flowers and fruit on the same branches*. I also cut from sprouts which grew up from the base of the tree, so that I might have all its characteristics, for I felt then that it would prove to be undescribed. I spent a long time in getting these specimens. I was then engaged with Dr. Shumard in the geological survey of the state. Drs. Shumard and Riddell were then with me,—they in a buggy, I on horseback—at Corpus Christi together, and we started next day for Austin, my press and plants in the buggy. They went on hurriedly, agreeing to change my plants, I arrived in Austin three days after them, and found my plants mostly spoiled. I saved only a few damaged specimens of *Hoopesia*. Familiar as I was with them in cutting, and again in my anxiety so few days after to select from the damaged specimens the best left, I do not see the possibility of any mistake; but intend to take the first opportunity to visit again the locality, and get specimens for the Academy and other public institutions. Trees of the *Hoopesia* grow on the banks of the Gulf, from one to two miles below Corpus Christi.

Drejera parviflora and *Morus microphylla* are admitted to be good species.

Juncus filipendulus has also, since Dr. Gray's criticisms, been decided by Dr. Engelman to be a good species. But he has changed the name, on the ground that my name is "inappropriate." My name was suggested by the thread-like hanging stems of its fruit and flowers. Even were the name less appropriate than it manifestly is, if botanists had a right to change names to accord with each compiler's sense of fitness, how many synonyms should we not have?

Juncus diffusissimus Dr. Gray says is *J. debilis*. Dr. Engelman has since decided Dr. Gray to be wrong in this, but I have not Dr. Engelman's monograph by me to refer to what he decides it to be.

Cyperus Heermanii is not referred to any other species, and

Eleocharis microformis, though said to be "near intermedia," is probably a good species also.

For want of books and material, as I have before remarked, I am unable to follow up these corrections further; but finding myself right in so many which I have been able to reconsider, I hope to be able to clear myself from some of the other charges in future.

Dr. Gray was particularly severe in his preliminary remarks. He accuses me of a "gross appropriation and suppression of the names of Nuttall and others, as recorded in a public herbarium." The laws of botanical nomenclature say "a name which has never been clearly defined in some *public journal or work*, shall be changed for the earliest name by which the object shall have thus been defined." See *Edinburg Philosophical Jour.*, 1863--4. Indeed, in the language of science, a plant has not been named until it has been described in some "journal or work." One may by courtesy adopt a name he finds on a label attached to a herbarium specimen; but if in his opinion, from the smallness or imperfection of the specimen, or from other reasons, he believes the interest of science would be served by the use of another name in his description it is his duty to do so.

I feel that I have been wronged by Dr. Gray's personal remarks in his review of my papers. Considering such a course out of place in a scientific discussion, I have avoided anything like retaliation. But I have thought it due to me as the author, and the Academy as the publisher, of the papers criticized by Dr. Gray, that no more errors should be laid to their charge than they legitimately deserve.

A New Classification of the North American FALCONIDÆ, with Descriptions of Three New Species.*

BY ROBERT RIDGWAY.

INTRODUCTORY REMARKS.

Having been engaged for a considerable time upon an investigation of the North American Falconidæ, I have found it necessary to arrange the sub-families with their sections, the genera and their subgeneric divisions, in a manner somewhat different from the classification usually adopted. The following scheme is intended to express the arrangement that I have been led to make, as the result of the study alluded to.

Of course, the classification presented is based entirely upon the external anatomy, and may, very probably, be found to differ from one founded upon the internal structure. As, however, the former is more convenient for practical purposes, and, moreover, there being no sufficient material at my command for a classification of the second kind, I trust that I may be excused for offering one based upon comparatively artificial characters. In the descriptions, every available character has been used, it having first been traced through the group to test its importance.

The present paper is intended as a preliminary to a "Monograph of the North American Raptores," now completed, and soon to be published, in a volume of the series of reports of the "U. S. Geological Exploration of the 40th parallel," under the direction of Mr. Clarence King. This work is intended to embrace full descriptions of all the species of the order belonging to the fauna of North America,† their differences from any analogue of South America or Europe being expressed by a diagnostic table; in which manner are also distinguished all the closely-allied species. The different stages of plumage of each species are elucidated in detail, and all doubtful questions as to the relationship of allied forms or the validity of others are fully discussed, and the complete synonymy given in full.

* One, however, belonging only to West Indies.

† As defined by Prof. Baird.